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PHYSIOLOGICAL AND PSYCHOLOGICAL FACTORS IN SEX BEHAVIOR

By

S. Bernard Wortis, Gregory Bateson, William E. Galt, Morris Herman, Alfred C. Kinsey, and William C. Young

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CONTENTS

PAGE
Introduction to the Conference. By S. Bernard Wortis 605

PART I: ANIMAL SEX BEHAVIOR

Animal Endocrines in Relation to Sexual Behavior. By William C. Young 609
Sex Behavior in Primates. By William E. Galt 617

PART II: HUMAN SEX BEHAVIOR

Sex Behavior in the Human Animal. By Alfred C. Kinsey 635
Aberrant Sex Behavior in Humans. By Morris Herman 639
Sex and Culture. By Gregory Bateson 647

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INTRODUCTION TO THE CONFERENCE

By S. Bernard Wortis

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Attitudes and traditions in our society, with regard to sex behavior, are derived from needs and ideas that have served man at some time in his development. Frequently, some of these attitudes and traditions become codified in law and, subsequently, a frozen segment of our mores. This conference will bring forth new material and point to correlations that emphasize the need to change our attitudes regarding human sexual behavior. One hopes that it may help to modify our laws in these matters.

Sexual function in man has come to be dominated by his mind. In lower animals, the endocrine secretions play a most important role in sexual behavior. In infra-human primates, the endocrine factor is less controlling. Sexual behavior of the monkey and the chimpanzee is modified by factors concerned with personal dominance, aggressiveness, and the individual's status in his social group. In man, these social, cultural, and personal psychodynamic factors have become preeminent. This "encephalization" of sexual function has developed, in evolutionary time, to the point where some endocrine secretions are of secondary importance to the mind. Today, we recognize that psychologic-cultural conditioning factors play a vital role; that sexual attitudes are developed in early life (before the child acquires specific factual knowledge) and are important in the psychosexual adjustment of the adolescent and the adult. We must, however, modify our ideas concerning normal patterns of sexual behavior.

Biologists tell us that neuromuscular patterns of bisexuality are present in the nervous systems of primates and other mammals. Even the endocrine patterns of humans show "male" and "female" sex hormones to be present in every person. Moreover, variations in sexual behavior, such as mouth-genital contact and sexual contact between primates of the same sex, have been observed in animal studies. Social taboos have strongly colored the attitude of our society to such experiences in man, despite the fact that such activity is usual in primate mammalian sex behavior. It is time that our society modified its attitudes to variations of the sexual impulse and tempered its laws relating to these matters with biological and psychodynamic wisdom. Much of the evidence collected in these papers will provide the basis for such orientation.

PHYSIOLOGICAL AND PSYCHOLOGICAL FACTORS IN SEX BEHAVIOR

Part I

ANIMAL SEX BEHAVIOR

ANIMAL ENDOCRINES IN RELATION TO SEXUAL BEHAVIOR

By William C. Young

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What is unknown about the endocrines and sexual behavior in mammals still exceeds by far what is known. It was only 10 years ago that the view of most endocrinologists was confined to the generalizations that the manifestation of male sexual behavior is stimulated by androgens, and that female sexual behavior is stimulated by estrogens. Much has been learned during the decade since 1936, but quite as important for the future, it seems to me, is the acquisition of a sense of direction. It is this theme that is to be developed in what follows.

The relationship of the endocrines to sexual behavior has been studied, in males and females, in a variety of mammals: rats, mice, guinea pigs, and hamsters, among the rodents; rabbits, cats, sheep, horses, cows, and dogs, among the larger domesticated mammals; and the *Rhesus* monkey, bonnet macaque, and chimpanzee, among the laboratory primates.

As far as the male is concerned, but few complications have thus far arisen. Agreement is general, among those whose work Beach[3] has reviewed, that the sexual responsiveness of castrate individuals is restored by the administration of androgenic substances; that the sexual excitability of sluggish individuals is increased by similar treatment; and that prepuberal rats likewise respond to androgenic action.

In the female, the situation is more complex. The results from experiments on the large domesticated, and the larger laboratory mammals, which are reviewed elsewhere,[36] have led to the opinion that mating behavior in the female is normally stimulated by such naturally occurring estrogens as estradiol or estrone, although in some members of this group, notably the sheep, a complete physiological response has not been obtained regularly, and the conclusion was reached by Cole, Hart, and Miller[11] that either a proper balance of hormones was not attained or unknown factors were involved.† In laboratory rodents such as the rat, mouse, guinea pig, and hamster, mating responses similar to those shown prior to ovariectomy have been experimentally produced only when a priming or conditioning injection of an estrogen was followed by the injection of a small amount of progesterone.[12, 7, 39, 2, 27, 16]

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† It has recently been reported, however (**Fraps, Schott, Simmons, & Phillips.** Abstract, Proc. Am. Soc. Zool., Anat. Rec. **96:** 74. 1946), that, in the sheep and goat, progesterone has the effect of terminating estrous periods induced by estradiol benzoate, and that the inconstant results obtained by other investigators may, perhaps, be explained by the presence of functional *corpora lutea* which developed following the injections of gonadotrophins that had been made in attempts to induce estrus.

An additional and previously unreported fact was revealed while the experiments which led to this latter conclusion were in progress. The sexual behavior pattern, as seen in the guinea pig and rat, may have as many as three components: the mating response, a male-like mounting activity, and a certain amount of running activity. It is known for the mating response in the spayed rat, and for the mating response and male-like mounting activity in the spayed guinea pig, that a single treatment with estradiol benzoate, or estrone followed by progesterone, is sufficient to induce these behaviors in an amount comparable with that shown prior to ovariectomy. A normal amount of running activity, on the other hand, has not been produced by this treatment. This has been accomplished only when small quantities of estrone were taken in the drinking water,[25] or administered daily in the form of injections of the aqueous hormone,[26] or when pellets of this hormone were implanted subcutaneously.[40] In the case of this component of the sexual behavior pattern in the rat, estradiol benzoate was but slightly effective (a single injection was entirely inadequate), and progesterone appears to have been without any action.

To what extent can other endocrine substances or synthetic hormones be substituted for the naturally occurring hormones? As far as I have been able to ascertain, experiments in which efforts have been made to find substitutes for estrone in the induction of spontaneous running activity, have been performed only by Hemmingsen and Krarup.[19] The synthetic hormone they used (9:10-dihydroxy-9:10-di-n-propyl-9:10-dihydro-1:2:5:6 dibenzanthracene) raised the level of activity well above that shown by the ovariectomized rats they injected, but there is nothing to indicate whether or not the level shown prior to ovariectomy was restored.

In the case of the mating response, stilbestrol[13] and triphenyl ethylene[28] can replace the naturally occurring estrogens; testosterone propionate can, to some extent, substitute for estrogen in rats[3]; and desoxycorticosterone acetate,[32] pregneninolone,[31] or an aqueous extract of the adrenal cortex can take the place of progesterone. However, in every report in which the point is covered, the naturally occurring hormones seem to have been more effective.

At least two investigators have shown that the frequency with which male-like mounting activity is displayed is increased by treatment with androgens,[1, 3] but in experiments which involved the induction of mounting activity by a single injection of gonadal hormones, the androgens used by Young and Rundlett[39] did not have a synergistic action comparable to that of progesterone.

The conclusion to be drawn from these experiments is that there are substances which come close to substituting for the naturally occurring estrogens and progesterone, in the production of the mating pattern characteristic of the individual animal. That the substances which have been used may substitute completely for the naturally occurring ovarian hormones, is a question which I feel is still unanswered.

Thus far, it has been shown that certain gonadal hormones are active in the production of sexual behavior. In moving beyond this point, I will elaborate somewhat on an aspect of the subject which in the aggregate has received considerable attention, but which nevertheless is in need of further, and a more systematically organized, study. Although the rapidly accumulating literature has never been reviewed, it is common knowledge that members of a species vary greatly in the nature and extent of their responsiveness, sensitivity, or reactivity to hormonal substances. This is as true of the behavioral responses as it is of the tissue responses. As the sexual behavior of the laboratory and large domesticated mammals has been studied, very great differences have been found in the qualitative and quantitative aspects of their behavior. Many of us have sought to correlate these differences, to ascertain if an animal in which the mating reactions were especially intense was also active in mounting other animals,[37] or if a prolonged period during which the female would accept the male was associated with a great amount of running activity.[18, 19] It cannot be said that such correlations have been found. Efforts have been made to ascertain if any relationship exists between the amount of estrous behavior and the number of maturing follicles[38, 40] and, in the case of two chimpanzees displaying contrasting extremes of behavior, the amount of excreted estrogenic substances was assayed.[15] However, in neither set of observations was any correlation shown.

Tentatively, the conclusions have been reached for the lower mammals[40] that "the mating response, the male-like mounting activity and the running activity are stimulated by the cyclically produced secretions of the ovaries rather than by a single substance. But in addition to the factor of a multiplicity of hormones having a selectivity or specificity of action, the situation is complicated by the circumstance that there is not a common threshold or sensitivity for the three parts of the pattern. Many observations suggest that each of the mechanisms mediating the three parts of the total pattern responds in its own manner and that the behavior which an animal displays is more strongly influenced by the sensitivity of these mechanisms to hormonal substances than by any quantitative fluctuations in the amount of secreted substances, provided the threshold of stimulation has been reached."

This inclination to focus attention on the sensitivity or responsiveness of the tissues on which the endocrines act as a central problem, impels us to seek factors which are responsible for the differences in sensitivity. This, it would seem, is one of the directions in which we must move if the basis of qualitative and quantitative differences in sexual behavior patterns is to be found.

The possibility that genetic factors may have much to do with the character of the response is the first to suggest itself. Although evidence for the existence of genetic factors within a species is still not extensive,[29, 10] no one would question the suggestion that the behavior patterns displayed by two species such as the rabbit and cat have a genetic basis. Proof is lacking, but it probably is no less reasonable to suppose that genetic factors operate within a species to produce strain differences, such as those described by Hauschildt and Evans.[17]

There is ample evidence for the existence of a relationship between the age of the animal and its responsiveness to hormonal substances, which comes from the work of those who have studied tissue reactions and those who have studied behavior,[20, 22, 23, 24, 30] and from older articles cited by Young.[36] For the most part, though, in the laboratory rodents, the changes which are in the direction of an increasing sensitivity seem to be confined to the first month or two of postnatal life. Thereafter, through the tenth month in the rat, and the second year in the guinea pig, no further changes have been detected.[35] It remains to be shown, therefore, for the period when reproductive activities are possible, and later, that age is related to the sensitivity to gonadal hormones and thereby to the character of estrous behavior.

Seasonal fluctuations in sensitivity of the tissues to gonadal and hypophyseal hormones are asserted to occur,[6, 9, 14, 21] but there is no very satisfactory evidence that the sensitivity to hormonal stimulation, involving behavioral responses, varies similarly.

The relationship of the endocrines to the problem of sensitivity needs clarification and further study. Hypophysectomy has, in general, been followed by difficulty in obtaining estrous responses, although hypophysectomized rabbits, dogs, cats, and rats have been brought into heat by replacement therapies. The high percentage of failures, and the weakness of the responses when estrous behavior was obtained, could be attributed to the severe general effects of the operation. The procedure, moreover, does not enlighten us with respect to the effects of specific endocrine organs such as the adrenal gland, the thyroid, and the gonads themselves. As far as the adrenals are concerned, the most suggestive studies are those of Bourne and Zuckerman,[8] in which evidence is cited for the action of the adrenal cortex in lowering the threshold for estrogen stimulation, as measured by changes within the vaginal epithelium. Tissue sensitivity has also been said to be modified in experimental hyper- and hypotension.*

Of the investigations dealing with the relationship of the endocrines to the sensitivity or responsiveness of the elements mediating behavioral changes, studies of the gonads have been the most numerous. What is clearest is that, during the prenatal period and much of postnatal life, the gonads are without any detectable effect. Not only are the development and maintenance of a normal sensitivity to estrogen independent of ovarian action,[35] but the injection of large quantities of estrogens and androgens during early postnatal life actually prevents the acquisition of a responsiveness to the estrus-inducing hormones.[33, 34] In a confirmation and extension of this conclusion, Beach[5] was able to show that the differentiation and development of the structures controlling sexual behavior are possible, even under the conditions of a congenital absence of gonadal tissue. Such being the case, we are compelled to look elsewhere than to the gonads for the factor which is responsible for this change in sensitivity. Moore[22] has postulated that sexual differentiation is under the control of genetic sex-determining factors, rather than of gonad-secreted hormones and, without denying a later activation to such hormones, it is suggested that these unidentified non-gonadal factors may account for many of the differences between individuals which are encountered everywhere. The possibility that such an inherited sensitivity can be modified, or influenced by physiological as well as by environmental factors, must, of course, be studied even more carefully than has been done. However, with excellent end-points available in both males and females, this should not only be practicable, but should also prove a fruitful field of inquiry.

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* Two recent abstracts contain information bearing on this point. **Fleischmann** (Fed. Proc. **5** (2): 28. 1946) notes that the ability of estrogen to modify certain metabolic changes in immature chicks is neutralized by treatment with thyroxin, whereas estrogenic effects on the structure of the oviducts are unaffected. In an investigation (**Young, Webster, & Innes.** Abstract, Proc. Am. Soc. Zool., Anat. Rec. January, 1947; in press) in which the sensitivity of the behavioral reactions was tested under the conditions of acute hyperthyroidism, the responses given by ovariectomized guinea pigs which survived the treatment were quantitatively and qualitatively similar to those shown prior to the injection of crystalline thyroxin. In three older studies (**Reiss & Perény.** Endokrinologie **2:** 181. 1928. **Van Horn.** Endocrinology **17:** 152. 1933. **Meyer & Wertz.** Proc. Soc. Exp. Biol. & Med. **38:** 843. 1938), the responsiveness of the vaginal epithelium to estrogens was shown to be lower in ovariectomized rats to which thyroid preparations had been given. In a third recent study, **Langham & Gustavson** (Fed. Proc. **5** (2): 143. 1946) found that injections of dl-thyroxin into ovariectomized rats for periods up to 10 days greatly decreased the sensitivity of the vaginal epithelium to constant doses of estrone, and that thyroparathyroidectomy and thiourea treatment had the ultimate effect of increasing sensitivity.

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SEX BEHAVIOR IN PRIMATES

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INTRODUCTION

The study of sex behavior in infra-human primates offers a fertile field of investigation. Monkeys and apes are relatively so close to man, from the point of view of evolutionary kinship, structural organization, and functional capacity, that significant findings in regard to the psychobiological interrelations of sex behavior at this level should provide valuable leads and insights in studying human sex behavior and its dysfunctions. Furthermore, the sex behavior of the sub-human primate, unlike that of man, is relatively direct, frank, and free of self-conscious attributes. It can, therefore, be studied under more or less controlled, experimental conditions.

The study of socio-sexual behavior and interrelations in these animal types is, however, not without its inherent difficulties. Several general lines of approach have been followed in studies dealing with this topic, but greatest emphasis in the present review will be given to findings which have resulted from investigations of sex behavior where the animals live under laboratory conditions, and where the answer to a specific question is sought, other variables being controlled as nearly as possible. It should be borne in mind that, in such studies, the animal is necessarily placed in a socially, sexually, and bionomically artificial situation. Conclusions drawn from the results must, therefore, be tempered by these considerations. Certainly, such conditions as captivity, segregation, and the stimulus of an observer intensify the primary sexual activity of infra-human primates.

SEXUAL CYCLE IN INFRA-HUMAN PRIMATES

The sexual cycle in the chimpanzee, and in some types of monkeys, lends itself particularly well to study, since it contains several easily observed phases. In the chimpanzee, the following macroscopic phases are discernible: menstrual bleeding; a postmenstrual condition, which is followed by tumescence of the genital area; a phase of maximal swelling, followed by detumescence; and a premenstrual phase, in which the sexual skin is quiescent. The sexual cycle in the monkey averages 28 days, while the median for the chimpanzee cycle is 35 days.[40] The longer cycle in the chimpanzee is due to a greater length in the pre-swelling and swelling phases. The combined length of these two phases in the chimpanzee is about 25 days, and is more than twice the length of comparable phases in the monkey. Evidence indicates that the swelling and coloration of the sexual skin in monkey and chimpanzee is caused by the female hormone, estrin. Ovariectomy performed on monkeys during the phase of maximal swelling results in prompt subsidence of the sexual skin, while experimental injection of estrin into ovariectomized monkeys produces the characteristic enlargement and coloration.

TIME OF OVULATION

Ovulation occurs in monkeys at the time of maximal genital swelling, about mid-way between two menstrual periods.[1] Elder[18] has demonstrated, using the method of experimental matings, that, in the chimpanzee, pregnancy results only when copulation occurs during the last few days of maximal genital swelling. Presumably, then, ovulation is limited to this period, and is followed rather promptly by detumescence, as is the case in the baboon and in the monkey.[41, 42, 44] Finch, Yerkes, and Elder,[20] using the Burr-Lane-Nims electrical potential technique, tested a female chimpanzee throughout two sexual cycles and found a maximal potential, presumably indicating ovulation, on the 17th day of each cycle, which was within the last two days of maximal swelling. Additional evidence on time of ovulation in chimpanzees is provided by Young and Yerkes.[40] These investigators examined the ovaries of four adult chimpanzees of known genital status and found that ovulation occurred shortly before detumescence.

SEXUAL RECEPTIVITY

Early literature on the sex behavior of infra-human primates advanced the view that they differed from many other mammals, and were similar to man, in that the male and female may mate at any time during the sexual cycle, instead of only during brief cyclic periods of estrus. Maslow[25] explains this appearance of copulatory behavior throughout the sexual cycle of the monkey as being due to the superimposing of the factor of dominance, which has, in these animals, become closely interrelated with the sexual response. He noted a qualitative difference between copulatory behavior in monkeys, depending upon whether the female was in estrus or in a physiologically non-receptive phase of her cycle. Copulation during the period of estrus was more apt to be of the functional type, whereas at other times it was more likely to be of the dominance type. Yerkes[32] holds that a basic cyclicity of behavior prevails in the sexual life of infra-human primates, and that this cyclicity is obscured by dominance and other factors of a socio-sexual nature.

There are definite indications, certainly, that sexual behavior in monkeys and apes is powerfully oriented in regard to the period of female estrus and ovulation. Ball and Hartman[1] have shown that, in the monkey, there is an intensification in copulation and other sexual activity immediately preceding ovulation. Zuckerman[42] has likewise observed that male baboons show a preference for females in which the sexual skin is swollen. On the basis of the observation of some six hundred controlled matings, each of which represented a definitely prearranged experiment with two subjects of known history, sexual status, and relationship, Yerkes,[32] and Yerkes and Elder[19, 36, 37] conclude that the female chimpanzee is non-receptive except during the swelling phases of her sexual cycle. They report that copulation in typical and complete form among mature and congenial animals occurred only during estrus. Sexual desire in the male was also found to be correlated with the sexual status of the female. It is lowest during the menstrual and adjacent cycle phases, and is highest during the phase of maximal swelling. The number of copulations observed during maximal swelling was twice as great as during the remainder of the sexual cycle, although the maximal swelling phase is not more than one-third the duration of the total cycle. Behavioral data indicated that, when the associated female was in other than the maximal swelling phase of her cycle, the male's desire for copulation was weak, and that if copulation occurred it proved to be less adequately satisfying to him. The acceptability of the female to the male thus varies directly with her receptivity.

Varied socio-sexual factors have been found to influence the length of the period of the sexual cycle during which the female infra-human primate will copulate. As has been mentioned earlier, important among these is the factor of dominance. It appears that the more dominant the male, both individually and as a phylogenetic characteristic of the animal type, the wider is the range of copulatory behavior in the typical sexual cycle, and the greater is the tendency of the female to respond accommodatingly to the advances of the male, irrespective of her sexual status. Some of the other factors which have been found to be effective in bringing about copulation in chimpanzees at other times than when the female is typically receptive, are: prior isolation of the male from the female; lack of opportunity for the male to observe the sexual status of the female before being placed with her; immaturity, or lack of sexual experience, of male or female; generalized environmental excitement; or the presence of a human observer. Notwithstanding that the intrusion of such socio-sexual factors clouds the issue, Yerkes, on the basis of his wealth of observation of chimpanzee mating behavior, makes the following generalization: "Normally and typically, a multiparous female will mate with a male with whom she is intimately acquainted and on friendly or affectionate terms only during a few days of the period of genital swelling about and including the usual time of ovulation. At other times, the female as a rule is ignored sexually by the male, or, if approached and solicited, she avoids him."

COPULATORY BEHAVIOR

Behavior exhibited in the act of copulation is exceedingly variable in the infra-human primates. It not only varies from one species to another, but from animal to animal in the same species, and in the same animal during different phases of the sexual cycle. The dorso-ventral copulatory position, with the male covering the female, is the one typically assumed in these primates. Ventro-ventral mounting has been reported, however, to occur on occasion, both in monkeys and in the great apes. Yerkes has emphasized the qualitative differences in the copulatory act of the chimpanzee, depending upon whether it takes place during or outside the period of female estrus. If the female is fully receptive and sexually experienced, she usually takes the initiative and goes to the male when their access to one another is permitted. The male typically assumes copulatory posture and awaits the female's approach and presentation. Upon reaching him, the female crouches low or even flat upon the ground, with limbs flexed and genitals directed toward, and often pushed against, the male. Completion of copulation is often followed by mutual examination of genitalia, and by grooming.

During times when the female is not fully receptive, the male chimpanzee usually goes to the female, on their having access to one another. She may respond by presenting, but the presentation is high and differs markedly from the intermediate or low presentation characteristic of the fully receptive female in maximal swelling. It is usually not followed by copulation. Although the male is physically capable of forcing his sexual desires upon the female, regardless of her desire and sexual status, such behavior has been found to occur infrequently in chimpanzees if the mates are known to each other, are congenial, and if the male has had an opportunity to observe the sexual status of the female before being placed with her.

There are indications in the sex life of both monkeys and chimpanzees that the female has greater copulatory power than has the male. In his observation of free-ranging howling monkeys, Carpenter[12] found that, when a mature male was with a receptive female, copulation occurred at frequent intervals until the male was satiated, when, in many cases, he was promptly replaced by another. As many as three males were observed to be satiated in succession by one estrus female. Evidence of sexual satiation in the female was, however, not observed. Yerkes[35] contributes similar observations on the chimpanzee. He finds that, during estrus, the capacity of the female for copulation is almost unlimited, whereas the male is readily satiated, fatigued, or wholly exhausted by one female.

Males differ in their acceptability to the female as sexual partners. Carpenter[12] reports that the female howling monkey may refuse a given male and immediately accept another. Yerkes[32] found that such preferential behavior also occurred in chimpanzees, and that with a preferred sexual partner the period of receptivity of the female tended to lengthen. Factors in the male chimpanzee which appear to be positively correlated with his sexual acceptability are vigor, penis length, copulation time, and number of pelvic thrusts before orgasm. A high degree of sexual selectiveness, aggressiveness, or dominance on the part of the male made him less acceptable to the typical female studied. It is of interest, in this latter connection, that, in a recent study of factors responsible for a satisfactory sexual *rapport* between man and woman, Maslow[27] found that marriages were more satisfactory where the male and female were of comparable dominance-drive, or where the male was slightly more dominant. Successful marriages did not usually issue from the union of a very dominant and a very submissive individual.

RELATIONSHIP OF DOMINANCE TO SEX

It was early noted, in the observation of sexual behavior in monkeys, that characteristic responses of a sexual nature subserved functions other than that of reproduction. Studies by Hamilton[22] and Kempf,[23] in which sexually mature *Macacus* monkeys and baboons were placed together, showed that the female might assume the position of sexual presentation seemingly in an effort to appease an angry, aggressive male. Tinklepaugh[31] reports that sexual responses were frequently elicited, in the monkeys and chimpanzees which he studied, in situations where thwarting occurred, and that these responses seemed to be of a substitutive nature. Furthermore, such substitutive responses are not limited to the female of the species. A subordinate male monkey may also take the position characteristic of the presenting female in response to a dominant, aggressive male. Such behavior has been described by Zuckerman[42] in baboons, and was observed many times in *Macacus rhesus* and Sooty Mangabey monkeys, by Warden and myself, in experiments securing data on dominance behavior in these animals. Zuckerman regards sexual presenting of this type as an important means by which a subordinate animal adjusts itself to a system based on dominance.

In a series of studies, Maslow,[24-26] and Maslow and Flanzbaum[28] made a systematic investigation of the effect of dominance on the social and sexual behavior of various types of monkeys. It was found that, while presentation does occur more frequently in a subordinate than in a dominant animal, it is by no means the most constant characteristic of subordinate behavior. In fact, the subordinate animal may often completely fail to show this reaction. A sexual aspect observed to be much more highly correlated with dominance status was dorso-ventral mounting of one animal by another. Maslow found that the dominant animal in a pair was responsible for between 90 and 98 per cent of the mountings, irrespective of the sexual gender of either animal. As a result of his studies, Maslow concluded that dominance is an extremely important determinant of social and sexual behavior in the monkey.

Maslow made some incidental observations, which indicated that a female naturally dominant to the male with which she was paired would, during the period of estrus, become subordinate to the male. Not only did the male mount her at such times, but he also tended to assume dominance in other characteristics of their relationship. Zuckerman[42] has reported this same type of recession in dominance of a dominant female during estrus.

Yerkes and his associates of the Yale Laboratories of Primate Biology[17,30,33,34] have contributed the most extensive experimental work on the problem of the shifting of dominance with changes in the female sexual cycle. In these studies, attention was concentrated on forms and aspects of behavior, and especially on sexual responses, as elicited by an experimental food-getting situation. The technique was essentially the same in the several studies made. Two experimental animals were confined in a large cage and secured food either by way of a chute or by opening a box on a platform in front of the cage. The pairs of animals were usually caged together throughout a series of observations which continued for at least one complete sexual cycle of the female. Observations were made daily during the spaced presentation of ten pieces of preferred food. Since only one of the two subjects was able to secure food on a given trial, the situation afforded excellent opportunity for the exhibition of various degrees of dominance and subordination behavior.

The results of the several studies may be briefly summarized as follows: In the male-female pairings, the priority of response in the food-getting situation typically shifted from the dominant male to the subordinate female during the female's period of estrus or maximal genital swelling. With the onset of detumescence in the female, priority of response was again assumed by the male, and he controlled food-taking until the female again reached estrus. From his extensive experience in the observation of chimpanzee behavior, Yerkes describes the priority of response which the female exhibits in regard to food, when sexually receptive, as a "right" or "custom" of chimpanzee interchange which is tacitly accepted by both members of the pair. The interpretation is advanced that, in general, the assumption of priority of response on the part of the female, during the period of sexual receptivity, is in exchange for sexual accommodation of the male. This, however, may not be the effective causation in the specific case, as is shown in the results obtained for one female who, although habitually refusing to mate even during periods of maximal receptivity, nevertheless gained priority of response at such times.

In the female-female pairings, changes in response priority usually occurred during the various phases of the menstrual cycle, but the relationships were by no means as clear as with the male-female pairs. Sexual receptivity appeared to have an opposite effect upon behavior, depending upon whether it was the naturally dominant or the naturally subordinate member of the pair that was in this physiological state. The naturally dominant female tended, in estrus, to lose priority of response to the subordinate female, while the naturally subordinate female, when sexually receptive, ordinarily achieved priority of response over the dominant female. This latter relationship was shown more clearly in a later study in which ovariectomized females were paired with other females in order to eliminate cyclic sexual changes in one of the members of each pair.

An attempt to determine the physiological basis for the observed shifts in priority of response with sexual status has been made by Clark and Birch.[16] These investigators studied the effects of differential sex-hormone level upon the priority of response of a male-castrate chimpanzee, when paired in the food-getting situation with an unoperated male. They found that, under treatment with male hormone, the dominance of the castrate-male, *i.e.*, his priority of response in the food-getting situation, increased. Treatment with female hormone, on the other hand, produced such a decrease in dominance that the previously dominant castrate became subordinate to his partner. The effect of female hormone on the behavior of this castrate-male thus was seemingly opposite to the effect of female hormone (estrus) on intact females. Several experimentally testable hypotheses, which would explain this differential effect of female sex-hormone level on the socio-sexual behavior of the male-castrate and the intact female, are suggested, and it is indicated that further experimentation is going forward.

In the absence, as yet, of definitive experimental data, the following hypothesis seems to tally best with all observed facts. It posits the existence of two interacting factors—one, hormonal; the other, socio-sexual—operating in the changes of female response-priority with sexual status. The strictly physiological effect of female hormone is to reduce dominance status, as is demonstrated by the experimental findings of Clark and Birch. This action would account for the observed reduction in dominance status, during estrus, of the naturally dominant female in relation to the subordinate male or female partner—a result which has been noted in varied primate types by Zuckerman, Maslow, and Crawford. At the same time, however, a contraposed socio-sexual factor is operative. The perception, by the male, of full female receptivity has, in the chimpanzee at least, a powerful and independent influence on the male's behavior. Relationships in this anthropoid seem to prescribe, as Yerkes has indicated, that the subordinate female, although physiologically less prone to dominance during estrus, shall at such time assume priority of response. Behavior exhibited in this situation indicates that both the male and the female expect such a shift in priority to take place. As already mentioned, male and female patterns of sex response are not sharply demarcated in the primate. Female monkeys and chimpanzees often act like males, especially if they are the dominant animal of a group or pair. This pervasiveness of the male pattern in female responses would account for observations by Crawford, and by Nowlis, that the naturally dominant female ordinarily grants priority of response to a subordinate female when the latter is in estrus. The dominant animal is, in this situation, responding with the usual socio-sexual custom of the male.

The operation of the two factors outlined would seem to have unquestionable survival value for the primate, in view of Maslow's findings that a dominant monkey, whether male or female, is almost never mounted by a subordinate one, and Yerkes' observations that mating behavior in chimpanzees is mostly unsatisfactory and incomplete where there is a large discrepancy in the dominance status of the consorts. Both the hormonal and the socio-sexual control tend towards producing, in the male and female, a more equal dominance status during the period of mating. If it were not for the former, a naturally dominant female would tend to be sexually inaccessible to a subordinate male. Were it not for the latter, a naturally subordinate female would be so far removed in dominance status from a dominant male that effective mating behavior could hardly occur.

The hypothesis here outlined seems to derive indirect support from an investigation by Nowlis[29] on the relation of degree of hunger to dominance-subordination behavior in the chimpanzee. In this study, it was found that changes in the behavior of the dominant animal have much more effect on the food-taking of the subordinate member of a pair than have great variations in the intensity of drive of the subordinate. On the basis of these findings, it would appear that, in order for strictly hormonal modifications in the female to result in priority of response over a naturally dominant male, it would be necessary that an extremely great increase in dominance accompany an increased level of sex hormone. On the other hand, a much slighter change in the behavior of the dominant male would result in the female's priority of response to the food.

INNATE ORGANIZATION OF MATING PATTERN

Evidence indicates that the mating performance of males of subprimate mammals is innately organized, in reasonably complete form. Beach[2, 3] has shown that rats reared in isolation from weaning until maturity, usually mate as readily and as effectively as an experienced copulator when first placed with a receptive female of the species. As one ascends the evolutionary scale to the infra-human primates, evidence for the innate organization of the male's mating responses is less conclusive. Factors of learning and social interreactions of varied kinds appear to become increasingly important in determining the mating behavior of monkeys and apes. While there is some indication that the adult male monkey is capable of effective copulation on the occasion of his first contact with a receptive female,[8] this is certainly not true for the chimpanzee. This fact is stressed in the observations of Bingham,[8] and of Yerkes and Elder,[38] on the latter primate type. Although the male may have participated in types of sexual play prior to adolescence, and though the experienced female may attempt to assist him in his efforts, the male is not capable of satisfactory copulatory behavior on his first attempt and requires practice and experience to bring him to a point where he is a skilful copulator, completely acceptable to the female. The female chimpanzee, on the other hand, behaves wholly appropriately and effectively in her first mating experience. It has not been determined if the female chimpanzee raised in isolation would be able to respond adequately to her first sexual contact. It is certain, however, that the process of maturation, plus learning from social contacts and sexual play before puberty, enable the female but not the male chimpanzee to behave appropriately and effectively in the first mating attempt.

BISEXUAL BEHAVIOR

The occurrence of bisexual behavior has been found to be a characteristic of many animal forms. Beach[5,7] has reported many instances of this type of reaction in his series of studies on the sex behavior of rats. Male rats quite often mount other males and execute the entire copulatory pattern. Less frequently, a male rat will assume typical features of the female sexual response when mounted by another male. Female rats, though showing typical sexual receptivity when mounted by males during estrus, frequently respond to receptive females with the execution of the entire masculine copulatory pattern, or of certain elements of it. It is particularly interesting that the most effective stimulus-pattern for calling forth the male copulatory response in female rats is the sexually receptive female: *i.e.*, precisely the same stimulus that is most potent in calling forth this response in the male.

Bisexual behavior is by no means limited to sub-primate mammals. As indicated earlier, such behavior was observed to occur frequently in monkeys. It has also been reported in chimpanzees by Young[39] and by Yerkes.[33] The latter found that associated female chimpanzees both mounted, and were mounted by, their companions of the same sex. He calls attention to the fact that, when one female mounts another, it is the female in estrus that is mounted. It thus seems that in chimpanzees, as in rats, the receptive female is a potent stimulus for calling forth male copulatory responses in another female.

A study of the sex behavior of infra-human primates and of lower mammals, then, gives indication that the patterns for both masculine and feminine mating-responses are an innate part of the behavior-reaction of both male and female. A thorough understanding of this broad principle, as it operates in animal types so closely related to the human species, should do much to enlighten our socio-cultural attitudes toward so-called "homosexuals" and place this behavior deviation in a proper biological perspective. As I attempted to indicate in an earlier paper,[21] such experimental and observational data should make clear that the either-or type of sexual behavior demanded of man and woman by the mores of Western culture, under threat of severe penalty, is not in line with the trend of sexual adjustment as it has developed throughout biological evolution. In making its present stereotyped restrictions, based upon a dichotomous interpretation of the sex-response, occidental culture overlooks man's biological heritage, his physico-chemical make-up, and his natively endowed response patterns.

In reviewing the experimental studies of sex behavior in infra-human primates, one is struck with the continuity of this behavior with that of lower animal forms. There is nowhere a sharp break from earlier phylogenetic patterns. Though moderately diverse in expression, and though more dependent upon the process of learning, sex remains in primates, as in other animal types, on a manifestly instinctive level. It is uninfluenced by such factors as self-consciousness, or of symbolic processes in the form of mental associations and projections. The marked divergence of the sex behavior of man, when compared with that of other animals, is of particular interest to me because of my association with the phylobiological researches of The Lifwynn Foundation. In man, the biological function of sex has been largely side-tracked by affect-laden images, symbols, and projections. Through an inadvertence in his socio-symbolic evolution, man has in no small measure been betrayed, as it were, by the very symbolic processes that have so greatly enlarged his possibilities of mental adaptation and communication. Because of this *faux pas*, man has now come to mentalize the deepest and most spontaneous needs of his organism. In consequence, sex has, to an immeasurable degree, become lodged in man's head, and has correspondingly receded from his more basic, instinctive centers of function. The presence of the mother image, of dependence, transference, and sentimentality is due to the intrusion of affecto-symbolic trends that play a major role in the varied dysfunctions in man's socio-sexual behavior.[9,10,11]

The predominant intermixture of partitive, symbolic elements in the sex behavior of the hominid doubtless accounts for the fact that man is the only animal that typically mates without regard to biological fitness for reproduction, and irrespective of the organism's physiological needs. Certainly, the generally accepted conclusion that human sex behavior is uninfluenced by the periodicity of sexual receptiveness and desire would hardly seem warranted, in view of the demonstration of sexual cyclicity in the behavior of a primate so similar to man as the chimpanzee in structure, development, physiological processes, and in many important behavioral aspects. Indeed, on phylogenetic grounds, it seems not unlikely that such a cyclic rhythm would constitute a vital element in healthy human sex behavior, and that it is only because man is dominated by symbolic images and ideas of sex that he has lost touch with the deeper and more basic physiological rhythms which natively control sex reactivity in other animal forms.

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GENERAL DISCUSSION

Dr. Lawrence S. Kubie (*College of Physicians & Surgeons, Columbia University, New York, N. Y.*):

A conference such as this stirs into activity many questions which have been lying latent in my mind. Concerning the patient, careful stalking of an elusive answer to an elusive question, that of the ultimate basis and the ultimate mechanism of sexual need, the work to date has eliminated many candidates for this role, but does not elect any one. By this, I mean that it indicates the existence of a large number of body processes which contribute to sexual activity, but which are not the *sine qua non* of such activity; leaving unanswered the question as to what is the ultimate biochemical basis of sexual need and sexual behavior.

Perhaps I can make this question clearer by indicating, briefly, the state of our knowledge with respect to the other basic instinctual processes, such as respiratory needs, water needs, and nutritional needs. With these, it is clear that, in the so-called resting state of the body, the simultaneous asynchronous processes approximately balance each other. When deprivation occurs, sooner or later this deprivation synchronizes the biochemical processes of the body. This, in turn, creates a generalized tissue need throughout the body, which then must be translated into behavior. In the respiratory sphere, deprivation can last only a few seconds before anoxemia begins to involve all body tissues. Water deprivation takes somewhat longer, and nutritional deprivation still longer, but the basic process is identical in all three.

Is there anything comparable to this in the psychosexual sphere? Are there any biochemical processes which are an essential *sine qua non* for erotic needs? Has deprivation any comparable physiological role in sex? Are local vascular or secretory changes essential, or are all the local changes in the reproductive and genital systems secondary in importance to glandular disturbances (in the pituitary, or elsewhere), or to changes in the central nervous system? Are we in a position, as yet, to answer the positive aspect of these questions?

Dr. Margaret Mead (*American Museum of Natural History, New York, N. Y.*):

It would seem, judging from the material which shows correlations between "intelligence" and efficiency of mating behavior in males, and not in females, that there is a possibility that the wrong units of comparison have been used. If, for mating behavior, we were to substitute the unit, effective reproductive behavior, then we would compare courtship and effective copulation, for males, with courtship, effective conception, gestation, delivery, lactation, and postnatal protective care, for females. We would find, I believe, that the same sorts of relationship existed for females as for males. Choice of the unit called mating behavior is actually based upon an artificial extension, to a two-sex comparison, of the unit which is meaningful for one sex only.

Dr. Bela Mittelmann (*Cornell University Medical College, New York, N. Y.*):

Sexual (genital) behavior can be stimulated, or inhibited, or generally altered, by essentially non-sexual situations and experiences. I would like to cite two observations, one accidental and the other experimental.

I happened to be standing at the elephant's cage in the Central Park Zoo, one day during the War, when the air raid sirens were sounded. The mother elephant and the baby elephant were in the cage. The baby elephant sidled up to the mother. The mother elephant definitely gave the impression, by the posture she assumed, that she was sexually excited. This observation indicates that anxiety-producing stimuli, under certain circumstances, may lead to sexual excitement.

The other observation is of an experimental nature. Gantt induced an experimental neurosis in a male dog, with the conditioning method of exposing him to various stimuli beyond the point of the animal's ability to differentiate. Among other behavioral and psychosomatic symptoms that the dog developed in the experimental situation, there was, also, the phenomenon of almost continuous erection. Further, when the dog was given the adequate sexual object, a receptive bitch, the dog engaged in sexual intercourse, but the duration proved to be shortened. In other words, the dog developed the equivalent of premature ejaculation. In some instances, the duration was one-third of that shown by this dog before the development of the experimental neurosis.

PHYSIOLOGICAL AND PSYCHOLOGICAL FACTORS IN SEX BEHAVIOR

PART II

HUMAN SEX BEHAVIOR

SEX BEHAVIOR IN THE HUMAN ANIMAL*

By Alfred C. Kinsey
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The data that are presented here, on human sexual behavior, represent a limited portion of material accumulated in an extensive case history study which has been under way at Indiana University in the past eight years. The project is being supported by Indiana University, and by the Medical Divisions of the National Research Council and the Rockefeller Foundation. The study is based upon first-hand interviews with persons of wide social range, of all ages, and of a diversity of educational, occupational, religious, and rural-urban backgrounds. 10,500 histories are now in hand. The material presented in the present publication is particularly concerned with showing the correlation between human patterns of behavior and the basic biological picture which has been developed by the students of sex behavior in other mammals.

Information concerning the ontogenetic development of human sexual behavior has been particularly scanty, because of the difficulty of obtaining information concerning the early sexual activities of children. The data now available have been drawn, partly, from direct interviewing of younger children down to the age of three, where a special play technique has been developed for obtaining evidence, not only of the child's factual knowledge and of his overt experience, but also of his attitudes on sexual matters. Part of the information has been obtained from memories of adults concerning their own pre-adolescent experience, and part has been obtained from observations made by adults on early childhood behavior. It is now clear that sexual attitudes develop at an early period and considerably in advance of the child's acquirement of specific factual knowledge. Attitudes are subtly shaped by the child's younger associates, and by the reactions of its parents and of other adults. At an early age, the child becomes conscious that there are peculiar social values attached to sexual matters, and that certain types of behavior are accepted, while others are condemned. The adult patterns are first laid down in earliest childhood and, by the onset of adolescence, the individual has acquired the pattern which is peculiar to the particular social level in which he lives. Few persons materially modify their patterns during their subsequent lives. The adult thinks that he has consciously chosen his particular course of action, but, in reality, he has merely accepted the judgments of his fellows and a pattern which is long-standing in his community.

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* The detailed data on which this paper was based will be included in a whole volume on Sexual Outlet in the Human Male, which is now in the course of preparation.

Specifically genital play occurs in two-thirds of the pre-adolescent boys, and in about a fifth as many of the pre-adolescent girls. There is more homosexual than heterosexual play. When the play involves contact with an experienced individual, the pre-adolescent may be aroused to the point of an orgasm which is as intense and as complete as anything in the adult. The work with the other mammals has shown that the bases of a complete sexual response are present at birth, and the data on the human are definitely in accord.

In the human, there are many of the same fundamental differences between male and female sexual development and mature behavior that are found in the lower mammals. The responsiveness of the human male develops earlier than that of the female. By fifteen years of age, 92 per cent of the human males have experienced orgasm, but the female population is 29 years of age before a similar percentage has experienced first orgasm. The spread between male and female is much wider in the human than in other mammals. The increased difference in the human is a product of the cultural restraints which delay female sexual development perhaps ten or twelve years longer than is biologically necessary. There is considerable difference in sexual conditionability of human male and female, which is in accord with the work on lower mammals. Males are aroused by a longer list of psychologic stimuli, more readily conditioned by past experience, and more aroused by anticipation of further experience. Visual stimuli are more significant to the average male.

The sexual activity of the male is continuous from the time of the first orgasm, at least if that orgasm occurs after the onset of adolescence. Over 99 per cent of the males have regular, usually weekly to near-daily, sexual activity, from soon after the onset of adolescence until 45 years of age. Female sexual activity is often discontinuous, with gaps of months and years between periods of sexual activity. For a majority of the females, there is no particular disturbance because of lack of sexual outlet during such periods, whereas all males are disturbed when the continuity of their sexual outlet is broken.

Human society has been considerably concerned about the so-called perversions in human sexual behavior, particularly about mouth-genital contacts, homosexual behavior, and such interspecific activity as the farm boy has with farm animals. The specific record indicates that all of these activities are far more extensive in human behavior than has been realized, occurring in 40 per cent to nearly 75 per cent of large segments of the normal, socially well-adjusted population. No one has realized their wide occurrence, because the social taboos have kept them from public knowledge. The work on other animals now makes it clear that all of these activities are usual and normal parts of the mammalian pattern of behavior. Their extensive persistence in human behavior, in spite of our English-American legal and cultural codes, and abundant evidence that they would occur even more widely if cultural restraint were removed, bear further testimony to their basic mammalian origins. Psychologic studies of these problems have been misdirected. It is not a question of explaining why such things persist in the human animal, but a question of explaining how so many individuals come to avoid and restrain their biologically normal reactions. Further studies of mammalian sexual behavior should be of prime importance in elucidating many other aspects of human behavior.

ABERRANT SEX BEHAVIOR IN HUMANS

By Morris Herman
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According to Havelock Ellis, sexual activities, entirely and by preference outside the range in which procreation is possible, may fairly be considered abnormal, *i.e.*, they are deviations. In order to remain within the normal range, all variations must, at some point, include the procreative end for which sex exists. Of course, one should substitute heterosexual genital contact for the term, procreation, in this definition, since the latter term excludes the normal use of birth control measures. There are serious difficulties in attempting to divide sexual behavior into the dichotomies, normal and abnormal. In reality, there exist kinds of sexual behavior common to the great majority of individuals, and types of sexual behavior which are different in some form or another. The term, aberrant, which literally means straying or wandering away from a straight path, is a good term to apply to these deviations from the average. All too commonly, physicians and others judge these aberrations in sex behavior with inadequate information by their own standard of what is normal or average. It is most important that the knowledge of the average sex conduct of human beings should be widely disseminated, with particular emphasis on the broad variations encountered. Numerous anthropological investigations of the sex life of primitives, and the sex data of contemporary Americans gathered by Kinsey and his group, are exceedingly valuable sources of such information.

It is evident that the relationship of an individual's sex conduct to the mores of the group is a determining factor in the consideration of its aberrant quality. However, many sex acts, such as mouth-genital contacts, etc., are utilized by many as preliminary to heterosexual genital contact. Are these, too, to be considered aberrant? We think not. Aberrant sex behavior can best be defined as sex activity utilized by preference as an end-point in gratification, despite the opportunity and ready availability of heterosexual genital contact.

The term, perversion, as applied to sexual deviation, is mentioned only to be condemned. It conveys a moral judgment which, unfortunately, is already too deeply ingrained in social thought.

Aberrant sexual behavior may be divided into the following types:

(1) Inhibition of sexual impulses; (2) augmentation of sexual impulses; and (3) deflections of sexual impulses.

Inhibition of the sexual impulse is clinically manifested in many ways. An individual may feel no interest of a sexual nature, make no effort to arouse erotic desire within himself, and develop no physical evidence of arousal. Such a condition may result from organic factors, such as the normal process of aging and some endocrine disorders (pituitary underfunction, hypothyroidism, adrenal cortical disease, and sex gland deficiencies). However, many instances occur which are due to psychological factors. A patient has been studied in whom the development of sexual inhibition was related to marked hostility towards a dominant wife. This man was a shy, passive individual. He succeeded, by means of his sexual inhibition, in inflicting punishment upon his wife, who bitterly resented his lack of sexual interest in her. Such total inhibitions may also be a manifestation of a diffuse hysterical process. Frequently, the inhibitions of the sexual impulse are not total but partial, producing such manifestations as impotence or frigidity, premature ejaculation, and vaginismus.

Impotence and premature ejaculation are generally accompanied by great anxiety and distress. In such instances, there is discord between the man's sexual urges and his ability to execute the sexual act, and he usually complains bitterly about his symptoms. The attitude towards frigidity in women is changing. In the last two decades, frigidity was accepted as the normal sex behavior in women. However, the educational aspects of psychoanalysis have broadcast the viewpoint that every healthy woman should attain climax and orgasm. The result is that many women who would have been content with their incomplete sexual performances, if they were living twenty or forty years ago, are not so today. The physician must preserve a scientific attitude, and keep open the possibility that the absence of orgasm may be biological in some women and not necessarily conditioned by emotional factors. Each case must be judged on its merits, and all the biological and psychological factors must be evaluated.

Impotence and frigidity are frequently the result of unconscious conflicts and anxiety. In the analysis of patients presenting such symptoms, one often found the following mechanisms: unconscious fears of punishment; conflicting loves such as parental fixation, latent homosexuality, and narcissism; and conflicting hates or aggression projected onto the sexual partner. A patient has been observed who complained of frigidity, and related the fact that, while having sex relations with her husband, she feels herself reaching climax, but is frustrated in achieving orgasm by the sudden appearance in fantasy of the face of her brother. Further examination revealed the information that she had had sex relations with her brother, and orgasm when she was nine years of age. This was followed by a profound sense of guilt and fear of punishment. These emotional feelings were instrumental in blocking the occurrence of orgasm in her adult life.

Augmentation of sexual activity is known as satyriasis in the male, and nymphomania in the female. Here again, the problem exists of determining whether an individual is possessed of a greater biological sex drive (a constitutional phenomenon), or whether such heightened sexual activity is manifested as a result of displaced unconscious conflicts and anxiety. There occur individuals who are latent homosexuals and overcompensate for these unconscious feelings by excessive heterosexuality, as if to convince themselves that they are not homosexuals by constant repetition of the sex experience. This group is often referred to as Don Juans or Donna Juannas. In some instances, the psychological meaning of increased sexual activity is a drive for power, in which the individual uses his sexuality to ensnare and conquer. He or she wishes to demonstrate to himself or herself superiority and dominance. Sometimes, the unconscious factors that motivate such behavior are found to be marked aggression and hostility towards the opposite sex.

Recently, there has been described an unusual case of nymphomania, in a woman of forty-five years of age. For several years, she had been experiencing greatly increased sexual appetite, even to the extent of having spontaneous orgasms. The causative factor was eventually determined to be a small tumor in the paracentral lobule of the parietal lobe of the brain, which was producing these sexual symptoms by irritation of the area of the topical projection of the genital structures. The symptoms disappeared completely when the tumor was excised. Another similar case has been observed, in which the excessive erotic symptoms appeared to be the result of stimulation from an epileptic focus in the brain. The sexual feelings in this woman occurred suddenly and intensely, sometimes awaking the patient from sleep. There was no psycho-erotic fantasy preceding the sexual arousal. This woman would either masturbate to relieve her sexual tension, or would engage in sexual intercourse, sometimes requiring the services of ten or fifteen men in one night before her sexual tension was relieved.

The sexual impulse is presumed to be directed towards attaining heterosexual genital contact. However, it frequently strays and is deflected into other erogenous zones, before achieving genital climax. In some individuals, this *deflection* of sexual activity is the end-point in the process. The psychological sexual process is either abridged or deviated, in such a way that some special part of the process, or some object or action normally on its margin, becomes the chief focus of attention. What is to the normal lover of secondary importance, or even indifferent, thus becomes of primary importance, and may properly be said to be the symbol of the whole sexual process. There are recognized such deflections of sexual conduct as mouth-genital contact, anal intercourse, masturbation, fetishism, sadism and masochism, exhibitionism, voyeurism, and homosexuality. The literature on these topics is enormous, but only a few observations can be made at this time.

Masturbation, as is well known, occurs in a very high percentage of individuals during their early life. This is a normal phase of development. However, in some adults, despite opportunities, masturbation replaces genital contact as a source of gratification, and so becomes aberrant sex behavior. This is found, frequently, in shy, rather introverted characters, commonly referred to as schizoid. It occurs also in neurotic individuals who are unable to achieve genital sexuality. It was so common a manifestation in schizophrenics that the older literature described masturbation as an etiological factor in the production of this psychosis. The masturbation here prepresents a regression to narcissism. The following case illustrates the necessity for evaluating sexual behavior in the setting of an individual's personality: A highly intelligent nineteen-year-old boy, a college graduate, stated that he was nervous and fatigued, and he believed that these symptoms were due to his masturbation. He was aware of the fact that masturbation occurs widely, normally without any ill-effects. But he argued that his body reacted differently from those of other people. This boy was a schizoid character, who expressed his narcissism not only in masturbation symptoms, but also in fantasy formation and projection symptoms, as a result of which he believed that others could recognize his masturbatory activity by changes in the rate of growth of his facial hair.

In a general way, deflected sexuality may be considered infantile sexuality. The cause and nature of this substitution for adult sexuality constitute the main problem. This may be due either to an arrested development, or to a regression. Since the aims of a deflected sexuality are similar to those of infantile sexuality, the possibility for every human being to develop sexual aberrations, under certain circumstances, is rooted in the fact that he was once a child. In scanning the literature, it is evident that, in different societies and different cultures, these deflected sexual activities have been differently regarded than they are in our present culture. Under varied conditions, such as enforced abstinence in prison, the state of the rejected lover, during alcoholism or drug intoxication, and in the course of psychotic illness, a person may temporarily utilize non-heterosexual genital techniques to obtain sexual gratification. This fluidity of response should make one pause before drawing sharp lines between the normal and the aberrant.

The difficulty of assaying homosexual conduct is a case in point. Freud stated, "I have never yet come through a single psychoanalysis of a man or woman without having to take into account a very considerable current of homosexuality." In practice, we encounter homosexuals who have shown these manifestations since very early in childhood. This gives the impression that the predominant causative factor in these patients is constitutional or biological. These individuals usually are exclusively homosexual. Another group appears to develop following the transition stage of adolescence, in which homosexual conduct occurs, normally, in a high proportion. These individuals generally show, as adults, various admixtures of homo- and heterosexuality. Still another type consists of persons who have been able to repress their strong homosexual tendencies, only to have them produce certain personality traits such as excessive shyness with members of the same sex, Don Juan conduct, or an inability to achieve satisfactory social and sexual adjustment with the opposite sex. The psychiatrist has the opportunity of seeing these mechanisms in operation in the analysis of character disorders, and in the critical observation of the symptoms expressed in the psychoses and neuroses.

The problem of constitutional factors in homosexuality, as shown by physical deviations, has been discussed by Henry and Galbraith, who concluded that homosexual patients show constitutional deviations from the general average. These findings have been contradicted by the work of others. Moreover, one sees individuals with marked secondary sexual characteristics of the opposite sex, and even with varying degrees of pseudo-hermaphroditism, who are without demonstrable homosexual leanings. An interesting case is reported of a person raised as a woman, who had male pseudo-hermaphroditism. Despite the presence of testes and a predominantly male physique, but only a rudimentary vagina, this person had the female psychological and erotic attitude towards men. It is evident, from such data, that evironmental conditioning is of paramount importance in the choice of sexual object.

When we study social attitudes and the law in relation to sex offenders, we come to a field which is full of challenging problems. Here, law and medical science do not see eye to eye. The law entertains the general attitude that the child is asexual; that any sex behavior on the part of the child is necessarily the fault of someone else; and that the child is always an unwilling victim in any sex episode. This is not true. In some cases, the child is the aggressor. A woman patient related that she began seducing men when she was six years of age. Our society assumes that girls below the age of eighteen are without sufficient will to assent properly to sexual experience. Intercourse with such a female, not one's wife, can result in the arrest of the man for the crime of statutory rape, even though this was not against the desire of the girl. Our society is tremendously concerned with protecting girls from sexual advances by men, but is comparatively unconcerned about protecting boys from sex advances of older women. Many men have been arrested with the charge of indecent exposure of their private parts. Women, however, are almost never arrested on such a charge, even though it is known that such exposure has taken place. At most, they may be charged with disorderly conduct.

Despite the large amount of newspaper publicity generally given to sexual crimes, its incidence in relation to all crime is very low, about 4 per cent of all serious crimes against persons and property. There are about 1,500 arrests, annually, for sex offenses in New York City. More and more, we have to realize that it is not so much the sexual behavior *per se* that is indicative of criminal behavior, but the other aspects of personality reaction. The sexual habits of individuals charged with sex crime differ very little from those of sexual aberrants in the community who have never come in conflict with the law. It is not the sexuality that should be regarded as criminal, but, as in other crimes, the offenses against persons or property.

The problem of the etiology of aberrant sex behavior has been complicated by the efforts of those who attempt to find a single cause for these disorders. As a matter of fact, no simple formula exists. The aberrations of sex conduct result from numerous complex factors, interwoven in such a way that it is often exceedingly difficult to trace them. Cultural-social influences, genetic factors, endocrine disturbances, central nervous system disease, intelligence, constitutional factors, and developmental psychological processes are all important in the production of these deviations. Throughout this dissertation, there have been references to the effect of these factors in the production of sexual changes. The importance of the central nervous system is illustrated in the regressive forms of sexuality found in senility and frontal lobe disease; in erotomania released by irritation of centers by tumor or epilepsy; and in impotence caused by spinal cord disease.

The personality factors in the production of sex aberrations provide a wide field for study and research. In the psychoses, we frequently encounter regressions to infantile sexual traits, either actual or symbolic, such as exhibitionistic masturbation, oral and anal activity, homosexuality, and sadomasochism. In the neuroses, too, there are manifestations of sexual problems in a disguised symbolic fashion. Many sexual aberrations in the adult are the result of conflicts in early life, identical with those of the neurotic. However, they have been elaborated in a different way. In those deviations of personality referred to as psychopathic states, sexual aberration is often present as a result of fixation at some early level of infantile development. Genital sexuality was never achieved.

In conclusion, the psychiatrist brings to the problem of sexual aberrations a technique for the evaluation of personality factors, and a knowledge of emotional disorders from which he can draw comparisons and conclusions. Above all, the psychiatrist, from his clinical experience, is impressed with the variability of human nature. He is aware that, in his dealings with human beings, sexual problems need much more study from all angles. Finally, the psychiatrist must evaluate the theories of sexual conduct and misconduct in the laboratory of actual life.

SEX AND CULTURE*

By Gregory Bateson
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It is certainly too early to try to introduce rigor into those anthropological hypotheses which mention sex as a causal factor, or which seek to explain the diversities of sexual behavior by referring to the cultural milieu. Even the word, *sex*, is used by us in a series of different senses, varying from observable and definable copulatory behaviors to a hypothetical drive, or drives, which are believed to influence a very wide and undefinable category of behaviors. It is even doubtful whether we should, at this time, attempt to sharpen our definitions, and not rather wait until some clarity begins to appear, as we amass more data. Definitions and abstractions are, after all, only "right" or "wrong" in so far as they form part of hypotheses which experience can test. Such hypotheses as we have today, relating sex to culture, are still so vague that very much more exploratory work will be needed before the abstractions involved can be sharply defined.

There is, however, a serious drawback to such a *laissez faire* attitude towards theory. It is, unfortunately, easy to construct hypotheses with vague concepts, and such hypotheses are usually impossible to prove or to disprove. The current theories of personality and character formation already contain an excessive number of parentheses (compensation, bisexuality, etc.), any one of which can be invoked to explain why behavior in a given case does not conform to hypothetical expectation. This building-up of parenthetical variables has reached such a point that today it is almost unkind to demand of any theorist: "What conceivable fact could disprove your hypothesis?"

There are, however, two possible approaches which may be of use: not to introduce rigorous hypotheses before the science is ready for them, but rather to suggest the sorts of question which we ought to be asking; and to delimit the orders of hypothesis to which we should look forward.

The first of these approaches will only be mentioned at this stage.

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* The original paper, as read before the Conference, was illustrated by a film of Balinese trance behavior, and by a series of Balinese carvings. These materials were used to demonstrate the wide ramifications, in human behavior, of effects which are in part traceable to "sex." The present summary examines more fully than was possible at the Conference the theoretical implications of the sorts of data which were then presented.

It consists in asking metascientific questions about that order of hypothesis which would relate a concept or set of phenomena derived from one scientific field (physiology) to concepts and phenomena derived from another field (cultural anthropology). We are attempting to argue from a narrower sphere of relevance, the individual's internal environment, to a wider sphere which includes almost the whole of human behavior and the external environment. All such transitions from a narrower to a wider sphere of relevance are known to be fraught with difficulty, and we may expect *a priori* that very simple alterations in the narrower sphere will be represented by excessively complex changes in the wider. A small change in atomic structure may denote a total change at the molecular level. Similarly, even so simple a matter as a difference in physical stature might determine very complex differences of culture or society. Physiological sex is known to have causally powerful and complex ramifications within the individual, and, *a priori*, we may expect the social and cultural ramifications of this set of phenomena to be so complex that "sex" will almost cease to be a useful category for the ordering of phenomena at this wider level. Indeed, we know already that those social extensions of "sex" which anthropologists call the "family" and the "kinship system" are crucial to the whole of culture, in the sense that all behavior can be related back to these concepts, just as the same whole can be related back to hunger and the economics of food.[1, 2, 3] This fact, that the effects of any phenomenon within the narrow sphere ramify throughout the *whole* of the wider sphere, indicates that we may not make much headway in attempting to trace the manifold cultural expressions of physiological sex. It is possible, however, that we might make advances by an inverse approach: that, from cultural data, we might be able to derive hypotheses about the narrower physiological sphere. This inverse procedure has an advantage, in that our hypotheses are the more likely to be subject to experimental testing.

The second approach to hypotheses which will relate sex and culture, consists in asking what sorts of data anthropologists do, in fact, collect. This can be followed up with the question: "What

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[1] **Malinowski, B.** The Sexual Life of Savages. George Routledge & Sons, Ltd. London. 1929.

[2] **Malinowski, B.** Coral Gardens and Their Magic. 2 vols. George Allen and Unwin, Ltd. London. 1935.

[3] **Richards, Audrey.** Hunger and Work in a Savage Tribe. George Routledge & Sons, Ltd. London. 1932.

types of verifiable hypothesis can be suggested or tested by data of this kind?"

Actually, there appears to be considerable confusion among other scientists, and among anthropologists themselves, about the nature of the data with which the cultural anthropologist works. Therefore, this matter must be made categorically clear. We too often think that the abstractions which we draw are a part of the data from which they are drawn and regard ourselves as studying "culture," or "social organization," or "diffusion," or "religion," or "sex." The creatures which we study are talking mammals and, whether they be natives of New York or of New Guinea, their talk is filled with abstract terms. Thus, we easily fall into the fallacy of assigning a false concreteness to these same abstractions. It is, therefore, salutary, at times, to leave all these abstractions aside for the moment and look at the actual objective data from which all the abstractions are drawn.

There are, I believe, only three types of data in cultural anthropology:

(1) *An identified individual in such-and-such a recorded context said such-and-such, and was heard by the anthropologist.* More than half of all our data take this form, and our main effort in fieldwork goes into the astonishingly difficult task of collecting such items. We do not always succeed, for various reasons. Sometimes, the individual is imperfectly identified. We may have insufficient information about his past experience and position in the kinship system and social organization. Still more often, we may have only an incomplete understanding of the context in which he spoke. But this remains our ideal type of datum.

(2) *An identified individual in such-and-such a recorded context was seen by the anthropologist to do so-and-so.* Here again, the ideal record is not always complete. The identification of the individual and the recording of the context present the same difficulties as in (1), above. In addition, we face very serious technical difficulties when we attempt to record bodily movements. Even with photographic or cinematic techniques, this is almost impossible, and the record, when obtained, can only with very great difficulty be translated into a verbal form for analysis and publication.

(3) *Artifacts* (*tools, works of art, books, clothes, boats, weapons, etc.*), *made and/or used by such-and-such individuals in such-and-such contexts.* These are, in general, the easiest data to collect, and the most difficult to interpret.

There are, at present, no other types of objective data in cultural anthropology.*

From inspection of this list of types of data, certain traps in anthropological deduction appear. The most serious of these is baited with the temptation to confuse verbal with behavioral data. Objectively, we may know that an individual said such-and-such about himself, or about some other individual; but we do not know, objectively, whether what he said is true. The objective fact—the only basis upon which we can build—is that he *said* such-and-such. Whether his statement is true or false, must be immaterial to any hypotheses which we may construct, unless (as sometimes, though rarely, happens) we have other objective data bearing upon the truth or falsity of the original statement. The importance of this point can scarcely be overemphasized when we are considering the validity of hypotheses relating to sex—a matter about which human beings are not only reticent and dishonest, but even totally unable to achieve an objective view of their own behavior or that of others.

An example may help to make clear how the anthropologist must proceed in such a case, and how he may construct hypotheses without assuming the objective truth of the verbal datum. Let us suppose that the anthropologist hears and records, *verbatim*, the statement of a man who claims: "I copulate with my wife n times every night." This may be an important objective datum, because within it are implicit numerous indications about the psychology of boasting and the psychological role of sex in that man's life. The next question which the anthropologist asks himself will not be: "Is the man's statement objectively true?" He will rather seek for those data which will enable him to place the man's statement in the cultural setting. He will want to know whether boasting and the use of sex activity for the enhancement of self-esteem are culturally acceptable. He will want to know whether such behavior is felt to be aggressive, and against whom the aggression is probably directed, and so on. He will therefore note, first and foremost, who is present on the occasion, and how these other people react to the boasting. He will look to see whether any of those present are markedly superior or inferior in status to the speaker, and whether any women are present; and he may later try to draw comments from the bystanders, after the original speaker has left the group. But in all this, he will not be trying to verify the truth of the original statement, and he will not, in general, care whether that statement is true. At most, he may carry a little suspended query in the back of his mind, a note that this is something about which he does not know, just to remind himself that every hypothesis suggested by the recorded statement must be so constructed that the truth or untruth of the statement will be irrelevant to the hypothesis.

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* In defining the contexts of human behavior, many types of non-anthropological information may be necessary. For this purpose, the field anthropologist may have to borrow from almost any of the other sciences. In practice, the "context" of behavior is usually limited to a narration of those factors which the anthropologist deems important and is able to describe. Merely permissive circumstances are usually omitted. For example, meteorological and geophysical circumstances always play a part in permitting an interview to take place, but these factors will usually not be mentioned in the record, unless the anthropologist believes that the shape and content of the conversation was thereby affected. More serious is the common omission of physiological circumstances: the appetitive state of both subject and interviewer, and so on. It is usually impractical to record these in anthropological fieldwork. However, the related sciences of psychoanalysis and experimental psychology are already benefiting from their closer association with the physiological laboratory, and we should certainly look forward to a time when physiologists will collaborate in anthropological fieldwork and amplify the scope of the available data.

The cultural anthropologist, in fact, is in the peculiar position of studying mammals which talk, and it is necessary to underline this fact to the minds of those who study less articulate and, therefore, less deceptive creatures. The circumstance that our subjects can talk to us, and to each other, is the great advantage which we have over the animal experimenter. However, it is very important not to abuse this advantage. To avoid such errors, stringent precautions must be observed, and these precautions necessarily limit the nature of the hypotheses which we can construct and verify.

Another peculiarity of the data collected by cultural anthropologists is the extreme complexity of each individual datum. The requirement that each datum include full identification of the individual and description of the context, is perhaps never fully met in practice. The fact remains, however, that a very large number of circumstances are always relevant, in the sense that a small change in any one of them might reverse, or drastically change, the form of the behavior which we are recording. There is, therefore, almost no possibility of handling the data statistically. The contexts, the individuals, and the behaviors are too various for their combinations and permutations to be handled in this way. The unit data of which any sample is composed are too heterogeneous to be legitimately thrown together into a statistical hopper. Moreover, the data are not selected at random, but according to circumstances which are forced upon the anthropologist rather than contrived by him. Anthropological informants, of which we do not use very many, are not a random sample of any population. Rather, they are carefully selected and carefully trained individuals, and the characteristics (accuracy, intelligence, articulacy, special interests, special social status, etc.)

which make a man a good informant are not statistically normal in any population. Moreover, the selection is performed by the informants at least as much as by the anthropologist. The man who is in some way deviant, psychologically, sexually, physically, or by social experience, is more likely to want to talk to the anthropologist, and is likely to withstand such interviews with a minimum of boredom. The normal, non-deviant individual rarely, if ever, becomes a regular informant.*

The fact that our data are not suitable for statistical analysis means that they must be handled in other ways. This can be done, just because the unit datum is so complex. It is not necessary to discover hundreds of specimens of *Archaeopteryx lithographica,* in order to satisfy the scientific world that this creature existed and had a number of phylogenetically significant features. The existing samples, consisting of one nearly perfect skeleton, one imperfect skeleton, and one single feather, are more than sufficient, simply because an *Archaeopteryx* skeleton is a complex object. In the same way, the data of the cultural anthropologist, if they are a valid base for theory, are so because they are complex. "This given complex pattern of events occurred"; and this unique occurrence is one of the bricks which must form the material for our theoretical constructions.

This peculiarity of our data, like the unreliability of verbal statements, is a factor which must limit our hypotheses. Neither the single *Archaeopteryx,* nor any number of single specimens of different species, would suffice to demonstrate whether evolution is a continuous or discontinuous process, nor to answer the many sorts of questions which can only be answered by statistical analysis of random or representative samples. Similarly, anthropological data will not suffice to test hypotheses which would require statistical validation, and we must, therefore, avoid hypotheses of this kind. Ideally, we should concentrate upon those hypotheses of which it may be expected that the single exception will disprove the rule.

Within the limitations outlined above, cultural anthropologists have a vast mass of objective data, directly relevant to sex. We cannot report, of course, anything about frequency or characteristics of "normal" sexual intercourse, because such behavior is only accessible to observation under circumstances so exceptional as to preclude use of the term, normal, *e.g.*, copulation on orgiastic occasions, or specially staged for the observer and distorted by his presence, or included in traditional dramatic performances.

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* There is some variation among cultures in this respect. Among the Iatmul, where verbal articulacy is highly developed, my informants were certainly more culturally normal than among the Balinese, where verbal skill is rare.

On the other hand, we have a large mass of information about native notions and psychological attitudes towards sex behavior, and we have information about how these various native attitudes and notions compare, and fit in, with other ideas which the same natives have on other subjects, such as achievement, sadism, humor, prestige, caste, etc. We know a great deal about the stylization of the differences between the sexes, and the roles of the two sexes in daily life and parenthood. We have collected hundreds of fantasies about copulation, love, homosexuality, incest, and so on. Then again, we have a mass of information on the economic aspects of sex behavior: dowries, bride prices, affinal exchanges, prostitution, etc. And masses of gossip about so-and-so's reputed sexual or courtship behavior, data which reflect on the culturally conventional attitudes towards various types of sexual normality and abnormality, and data on the sanctions which the people carry out (or say that they would carry out) in certain types of deviant circumstances.

The problem is to introduce theoretical order into this confused jumble of data, arbitrarily separated from the remainder of our data by their evident relevance to the concept of "sex" which is derived from the physiological sphere of relevance.

Two generalizations can be drawn from all this material: first, that cultures differ markedly among themselves and, second, that a high degree of consistency obtains among the data on any one culture.*

These two generalizations suggest that the regularities which occur within one society† are of a different order from those which occur within an individual organism, and the matter can, perhaps, best be made clear by discussing this difference. If a biologist were allowed to make an exhaustive study of several different tissues derived from a single species of animal, and then were presented with another tissue differing from all those which he had so far studied, it would be almost impossible for him to determine whether this last specimen was or was not taken from the same animal species. The cultural anthropologist, on the other hand, if presented first with data upon several sorts of individuals in a given society, will probably be able to recognize that the data referring to other sorts of individuals in that same society do, in fact, have that provenience. Moreover, in their attempts to solve this problem, the biologist and the anthropologist will look for clues of quite different sorts. The biologist will look for characteristics of the cells so basic that they persist even through tissue differentiation. For example, he may attempt to count the chromosomes. The anthropologist, on the other hand, will look first for details of language and other very superficial learned characteristics, and if details of this sort are not available, he will look for more basic patterns and regularities which will be diagnostic of the *acquired* character structure of the individuals. It is significant that the biologists talk about "differentiation" of cells and tissues, while the anthropologists talk about the "acculturation" of individuals. The problem stressed by the biologist is: "How do genetically similar cells become different one from another, and how do they maintain these differences in spite of a homogeneous environment internal to the individual organism?" The problem for the cultural anthropologist, on the other hand, is: "How do individuals, who presumably differ among themselves in innate characteristics, become similar and remain sufficiently similar to understand each other, in spite of very evident differences in individual experience?"

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* Operationally, it is probably necessary to define a "culture" as an aggregate of collected objective data of the kinds mentioned above. This definition may later be amplified, if necessary, by inclusion of some references to the type of order imposed on the data by the scientist. If, however, we limit ourselves to the minimum operational definition, the demarcation between one "culture" and another will have to be defined in terms of causal integration. If we fall into the error of defining this demarcation in terms of homogeneity, saying that culture **A** shall be separated from culture **B** if the data included under **A** differ markedly from the data included under **B**, then we shall have great difficulty in dealing with the differentiation of occupational, age, and sex groupings within the single culture. Still more serious, the generalization in the text above will be a mere endowing of our data with a characteristic of our own operations. If, on the other hand, we delimit our cultures by saying, "Data shall be assigned to a single culture so long as causal interdependence among the data can be recognized," ignoring for the moment the problem of the delimitation of cultures in the time dimension and the related problems of culture contact, we shall, at least, postpone these troubles.

† Operationally, the "society" may be defined as an aggregate of those individuals actually mentioned in the data which constitute a single culture, *plus* those others about whom data can be presumed to belong to the same aggregate.

In fact, to the cultural anthropologist, man appears not mainly as a physiological mechanism, nor yet as a creature endowed with instinctive urges and innate patterns of response. He appears to us, above all, as a creature which *learns*. The fact of human flexibility under environmental experience determines the major focus of our scientific attention.

Let us now return to the problems of "sex." If learning is the basic concept for the cultural anthropologist, then we can take a first step in relating "sex" to "culture," by examining the relations between sex and learning, bearing in mind that the phenomena connected with learning are to give us the definition of those sorts of regularity and homogeneity which we observe to be characteristic of each single culture. This definition still remains to be drawn.

To the anthropologist, it appears that all human sexual behavior is, in some degree, learned. The human infant apparently develops, at a very early age, a considerable reflex equipment. Its genitalia are erectile in response to various physical stimuli, and this tumescence is early associated with specifically interpersonal stimuli. This reflex equipment is precocious in the sense that the neural connections are present before the infant has the muscular development necessary to put the whole mechanism to work.

In this, there is nothing peculiar about sexuality, and the same sort of precocity is recognizable in other sorts of behavior. The infant also shows what appear to be inherited reflex arcs for walking, swimming, arboreal suspension, and balance in an upright posture. For all of these activities, it appears that neural connections are established before the muscular development is adequate, and the rudimentary responses, which indicate the existence of these neural connections, fade out. A period of "latency" occurs not only in regard to sexual, but also locomotor behavior.*

Now, the crucial question about sexual latency is: "*Is it learned*?" Is the change from genital responsiveness to unresponsiveness to be ascribed to topological changes in the neural network, induced in that network by impulses which pass through it? The obvious alternative to such a hypothesis would be to ascribe the change in responsiveness to changes in the endocrine system and to hope that a fuller knowledge of maturation will enable us to account for these changes in endocrine balance without again being pushed back upon a theory of changes in the neural network induced by experience.†

From what little we know, it appears to me that we must assume that latency is learned, rather than due to a hypothetical endocrine change. The necessary shift in endocrine balance has not been observed,[4] and it appears, rather, that concentrations of androgens and estrogens in children's urine show a progressive rise through childhood to a peak at puberty. Moreover, the hypothesis that latency is learned will have the additional recommendation that it can be applied not only to sexual but also to locomotor latency, for which an endocrine theory can less readily be imagined.

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* We have, unfortunately, no comparative data about the occurrence of these various types of latency in different cultures, where different methods of handling, carrying, and exercising the baby occur. Even for sexual latency, the data are very poor, and it is perfectly possible that, in some cultures, the potentiality for genital tumescence does not disappear in childhood. It is, however, to be expected that, in all cultures, there is a period during which the child ceases to show specifically sexual desires directed towards adults.

† A third possibility (that all types of latency are due to topological changes in the neural network, but that these changes are a function of maturation and not brought about by the passage of neural impulses) would also be tenable. This hypothesis will, however, differ somewhat from conventional notions about maturation, in that it must account for the breaking or inhibition of previously existing arcs.

[4] **Neustadt, R., & A. Myerson.** Quantitative sex hormone studies in homosexuality, childhood, and various neuropsychiatric disturbances. Am. J. Psychiat. **97**: 542–551. 1940.

Granting that learning probably plays a part in causing latency, the next question must be: "What are the stimuli or contexts which determine this learning?" Here we know, from anthropological data, that marked differences exist between cultures. In American and English cultures, we know that, among adults, even the notion of infantile sexuality is strongly resisted; masturbation of the child by parents or nurses is strongly deprecated (and, therefore, probably accompanied by guilt reactions on the part of the adult, when it occurs); and masturbatory behavior on the part of the child is sharply discouraged. Therefore, for these cultures, we must expect that latency will be induced not only by experience of own muscular insufficiency, but also by positive extinction or inhibition of tumescence. In sharp contrast to this, we know of cultures in which masturbation of the child by the parent is common and not deprecated. Even among these cultures, we may expect sharp differences which will be significant for the character formation of the child. The reason given by Italian peasants for this masturbation of the child is "to put it to sleep", and we may presume that, in Italy, the child is given some sort of sexual climax or other satisfactory experience. In Bali, on the other hand, our observations show that the child is not given satisfaction and, instead of going to sleep, becomes more restive. Indeed, from the mother's evident enjoyment both of the baby's responsiveness and of the temper tantrum which often follows, it would appear that the purpose of the masturbation is rather to wake it up. It is easy to see that the Anglo-Saxon and Balinese systems of handling may induce latency, though very different types of latency in the two areas. What sort of latency, if any, is induced by the Italian system, is not so clear.

These contrasts indicate very clearly that the social contexts which accompany the onset of latency are important for sexual learning. They may determine the individual's attitude towards sexual behavior and the part which these behaviors will play in his character. The matter becomes still more complex when we go on to consider the culturally stylized sexual play, masturbation, and courtship behaviors of the latency period, and the rewards and punishments which determine the role of these behaviors in the individual's character. These experiences will label the sexual patterns as safe or dangerous, approved or disapproved, as important sources of self-esteem and prestige, or as important sources of sensual pleasure.

Still later, the onset of puberty can be seen as a further set of learning and character-forming contexts, and the problems are analogous to those which we discussed in connection with latency. Here, the case for ascribing the change in responsiveness to endocrine factors is perhaps a little stronger, but it is possible that the importance of these factors has been exaggerated. There are, in addition, a host of interpersonal and social factors which push the individual into puberty. The sexual initiative of other persons; the value which the individual himself has been trained to place upon sexual adequacy and sexual conformity; his desire to acquire the respect of his fellows of the same sex and of possible sexual partners; all of these, in addition to his endocrines, may push him towards puberty. Further, the psychology of that puberty, when attained, will be determined by the specific qualities of the latency which has been broken down, and by the dynamics of the beginning of puberty. The individual who is pushed into active sexual life, by his physiological needs, at a period when he still feels that social pressures are on the side of latency, will learn something very different from what is learned by the individual who enters upon sexuality in an attempt to conform to social pressures before he is driven to this by physiological need.

The role played by sexual behavior and experience in determining character structure, and the inverse role of experience in determining sexual behavior, could be elaborated almost *ad infinitum*. To relate human sexual behavior to learning is easy, though at every turn we come upon new problems and new hypotheses requiring data for their verification. However, there is still another order of hypothesis which must be considered before we can be said to have related sexual behavior to culture.

It was suggested, above, that the data on a given culture show an internal regularity, or consistency, not simply deducible from the operation which we performed in defining the limits of the single culture in terms of causal interdependence of all events within this margin. We noted, also, that there is a contrast between physiology and cultural anthropology in that, though both sciences deal with spheres of relevance definable in terms of causal interdependence of events within the sphere, the physiologist is preoccupied with differentiation, while the anthropologist is preoccupied with acculturation. Our next step will be to define more sharply these regularities within the single culture, and to relate them to the theory of learning.

What has so far been said about learning could be deduced from any learning theory, such as that of *association*, which will describe learning as the setting-up of a classification of perceived objects and events, linked to a classification of responses and to a rudimentary linear value system which will discriminate the pleasant from the unpleasant. Such a system will, for example, account for the simple forms of sexual symbolism. Balinese carvings, for instance, illustrate a large number of types of symbolic distortion of the human body. The breasts may be equated with buttocks; the head may be equated with male genitalia; the mouth may be equated with the vulva; and so on.* All of these distortions can be seen as due to simple associational learning.

We find, however, in our cultural data, something more than this. If we take the data from a given culture and sort them by subject matter, putting all the data which refer to sex in one heap, the data referring to initiation in another, the data referring to death in another, and so on, we get a very remarkable result.[5] We find that similar types of order are recognizable in every heap. We find that, whether we are looking at the sex data, or the initiation data, or the death data, the system of classification of perceived objects and events (the *eidos* of the culture) is still the same. Similarly, if we analyze the heaps of data to obtain the system of linked responses and values (the *ethos*) of the culture, we find that the ethos is the same in each heap. Briefly, it is as if the same sort of person had devised the data in all the heaps.

Two obvious hypotheses which might account for this finding can, I believe, be ruled out. We cannot assume that these ethological and eidological regularities are simply due to innate human characteristics, because very different kinds of ethos and eidos have been analyzed out of different cultures. And we cannot say that the ethological and eidological uniformities are due to the uniform working of peculiarities of the mind of the analyst, because in different cultures the same analyst obtains different results.

It would, I believe, be impossible to deduce these results, the uniformity within one culture and the contrast between cultures, from the simple associational learning theory from which we started.†

However, these ethological and eidological uniformities within the single culture, and the corresponding contrasts between cultures, are precisely what we would expect if, in addition to the processes postulated in simple learning, there is a carry-over from learning in one context which will influence later behavior in quite different contexts. Various theories of this type have been put forward[6, 7, 8] and, in general, the experimental findings indicate that some such postulate may be necessary even at the animal level. At the human level, the carry-over from one context to others can be demonstrated in the phenomena of "transfer" of learning, and especially in the experimental increase in learning proficiency from one context to another of similar formal structure.[9]

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* Specimens of these carvings, collected by the writer, were exhibited at the conference. The collection of about 1200 carvings has been deposited at the American Museum of Natural History, New York, N. Y.

† The logical proof of this assertion is, however, beyond my powers, and probably not feasible until the concepts of *eidos* and *ethos* and, indeed, the whole of *gestalt* psychology have been much more critically defined than is possible today.

[5] **Bateson, G.** Naven. Cambridge University Press. 1936. (This book is an experiment in analyzing a New Guinea culture, on these lines.)

Such a postulated carry-over from one context of learning to another will give us a theoretical system which will permit us to speak of changes in *character*, instead of limiting us to the mere addition or subtraction of associational links. We can very easily see how such a theory would give precision to qualities of the order of "optimism," "pessimism," "fatalism," "initiative," "level of aspiration," and the like, and lead us to expect that qualities of this sort, learned by experience in one sort of context, will be carried over into other contexts of very various types. This, I suggest, is the explanation of the ethological and eidological uniformities characteristic of each human culture.

We are driven, I believe, to conclude that what is learned in contexts associated with sex will be carried over into contexts associated with quite different spheres of life—initiation, death, trade, etc.—and that, *vice versa*, what is learned in these other contexts will be carried over into specifically sexual life.

Such a conclusion will reduce the title of the present paper to nonsense, by indicating that *sex* is scarcely a useful concept for the analysis of human cultures; a conclusion which was foreshadowed in our *a priori* metascientific examination of any attempt to relate phenomena in the physiological sphere of relevance to phenomena in the cultural sphere.

Our excursion into theory has not, however, been fruitless, because it has lent anthropological support to a type of hypothesis connected with learning, and this type of hypothesis is such that it can be tested

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[6] **Frank, L. K.** The problems of learning. Psych. Rev. **33:** 329–351. 1926.

[7] **Maier, N. R. F.** The behavior mechanism concerned with problem solving. Psych. Rev. **47:** 43–58. 1940.

[8] **Bateson, G.** Comment on **M. Mead's** The Comparative Study of Culture and the Purposive Cultivation of Democratic Values. In: Science, Philosophy, and Religion, 2nd Symposium. New York, N. Y. 1942.

[9] **Hull, C.** Mathematical-Deductive Theory of Rote Learning. Yale University Press. 1940. (This book gives experimental curves for increase in proficiency in rote learning, but does not deduce this increase from a postulate system.)

and made more precise by further anthropological work, and by laboratory experiments. In addition, we have demonstrated that, in the psychological analysis of anthropological data, it is not useful to classify these data according to the sorts of physiological need to which they appear relevant. It is, however, very rewarding to classify these data according to the formal characteristics of the contexts of behavior. It is important to note that the Balinese baby is subjected to the same formal sequence, both when the mother refuses to respond to its temper tantrum and when she cheats it of sexual climax, and that the mother's behavior in both these contexts is an effect of her own past character-formation, as determined by experiences similar to those to which she is now subjecting the child. From such a beginning, we can go on to look at other types of Balinese data, and recognize that the same formal sequence recurs in certain ceremonials in which young men attack a masked figure representing the Witch. They are powerless against her, and fall into a state of disassociation in which they symbolically turn their aggression against themselves, thus achieving an introverted climax.*

From such a systematic analysis of the contexts of learning and the native interpretations of context which are implicit in cultural data, we may hope to build a formal science of culture.

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* For photographs of the ceremonial, see **Bateson, G., & M. Mead.** Balinese Character, a Photographic Analysis. Special Pub., No. 3, N. Y. Acad. Sci. 1942.

GENERAL DISCUSSION

Dr. Lawrence S. Kubie (*College of Physicians & Surgeons, Columbia University, New York, N. Y.*):

Even if Dr. Kinsey's report did nothing more than place the facts of infantile sexuality on an unquestionable statistical basis, it would be a major contribution to our understanding of human development and human culture. Psychiatry and psychology will always be in his debt for this. I only wish that some scientific Supreme Court could order certain reluctant psychiatrists to listen to the recital of these facts, every morning, for a month, and perhaps again before they go to bed at night.

In a parenthetical remark of Dr. Kinsey's, there is an implication from which I would dissent. He implied that, when any behavior pattern is widespread among human beings, we need not undertake any "complicated analytical explanations" of it. I am sure that, on further thought, Dr. Kinsey will agree that this is not true. The physiologist does not feel that he does not have to explain the mechanism of the heartbeat merely because everybody's heart beats. Nor does the epidemiologist dismiss the problem of the common cold merely because everybody catches colds. Universality is not synonymous with normality; and our obligation to explain every variation of sexual conduct, whether heterosexual, homosexual, or any other form, is not lessened in any way by the fact that every form of sexual behavior is widespread.

Perhaps, it is worth while to emphasize here the fact that a great deal of the controversy about analysis, and much of the resistance and hostility to it, has arisen because Freud recognized the universality of deviant sexual impulses even in those who give to these deviant impulses no open expression. Certain aspects of analysis stand or fall on the validity of the hypothesis that deviant instinctual tendencies are present, in latent forms, in every human being, derived from the polymorphous infantile sexuality whose existence Freud postulated, and Kinsey has demonstrated, so brilliantly.

Freud related these basic observations to the development of the neurosis, indicating that it was the conflict between latent trends and the forces opposing these trends that splits the personality asunder and leads to psychopathology. It sometimes seems to me that Freud and our analytical colleagues occasionally forget the importance of the conflicts, and speak or write as though it were the latent trend which is itself abnormal and pathogenic. I believe that it would help to bring into harmony the formulations of the biologist and of the psychoanalyst, if we could agree that it is never the deviant drive, as such, which is abnormal, but rather the conflict which arises out of it, and the compulsive and obligatory quality which may attach itself to the drive. An obsessional furor can attach itself to heterosexual activity just as readily as to masturbation or to homosexuality or any deviant forms of sexual conduct. It is the development of this obsessional furor, plus the phobic exclusion of alternative outlets, which is the mark of abnormality, rather than any specific pattern of sexual behavior.

For these reasons, it would add immensely to the value of the observations made by Dr. Kinsey if we could know more about the psychological and psychopathological setting of these various forms of sexual behavior. To this end, it would be essential to gather statistically adequate random samples of each form of sexual behavior, and that these samples be studied intensively by individual psychiatric interviews and by a battery of psychodiagnostic tests.* This, in turn, would lead to much additional information as to the way in which the same individuals handle instinctual processes other than sex, particularly those having to do with food and fluid intake, with excretion, with exercise and sleep. The addition of such information as this would be of great importance for the knowledge of human nature.

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* **Harrower, M. E., & R. E. Steiner.** Large-Scale Rorschach Techniques: 419. C. C. Thomas, Springfield, Ill. 1945.

Rapaport, David. Diagnostic Psychological Testing **1:** 574; **2:** 516. Year Book Publishing Co., Chicago. 1945.

Dr. Eilhard von Domarus (*New York, N. Y.*):

In Dr. Kinsey's excellent presentation of man's sexual life, little, if anything, has been said about the influence of different hereditary backgrounds. Assuming that there are, for instance, cases of homosexuality primarily determined by environmental factors, such as of a boy expected to behave like a girl, there appear to be cases where no such environmental factors can be detected and the homosexual conditioning appears primarily, if not exclusively, due to a specific sex behavior determining chromosomal influence. In the latter case, no approval or disapproval of society would be of causative significance. Dr. Kinsey's opinion, with his wide experience in this field, would be extremely valuable.

Dr. Joseph K. Folsom (*Department of Economics, Sociology, and Anthropology, Vassar College, Poughkeepsie, N. Y.*):

Having followed some of the details of Dr. Kinsey's study, I am inclined to agree with him that his results are a fairly accurate picture of the actual behavior of his subjects. Those who have had closer contact with the research, or have themselves been subjected to it, will better understand what he means by the internal consistency of the results and the improbability of any substantial amount of faking.

Furthermore, I think that it is very important to have the kind of data which Dr. Kinsey is providing, even if they are not accompanied by the kind of analysis on the attitude level demanded by Dr. Bateson. We have never yet had, anywhere, such a volume of material on sex behavior. Most of our data have been gathered through questionnaires. This study has the advantage of a well planned, skilful interview technique, and the interviewers have been carefully trained.

Some question might arise as to whether the selection of the subjects has something to do with their personalities and attitudes, even though every socially and externally definable type of person is represented in adequate numbers in the sample. Yet, it must be noted that many of Dr. Kinsey's subsamples included practically the whole population of certain groups to which he spoke. Furthermore, even if it were true that those who volunteered as subjects for such a research, or attended preliminary meetings where the subject was discussed, have personal behavior characteristics different from the average, this would hardly affect the differences which were found between different categories of persons. Moreover, the total is so large that plenty of any given type of person can be found in it, and there is abundant opportunity for checking suspected biases. Many of the subjects were picked up, of course, by casual individual contacts.

From the beginning, I wished that Dr. Kinsey might secure more data of the attitude type, and it appears that his study has already moved considerably in this direction. I hope that, eventually, he may classify and compare his cases, not only by mechanical categories of age, sex, education, etc., but by cultural categories which the anthropologist has found significant. For example, what are the characteristics of Italians, of members of very liberal, religious or other ideological groups, of persons who belong to the upper class according to Warner's community and prestige definitions, not merely according to mechanical definitions of wealth, educational level, etc.?

I am inclined to think that, in the wealth of data that Dr. Kinsey will eventually have, there will be found answers to most of the questions which the anthropologist might ask. I hope that all these questions will be explored, or the data made available to the anthropological, culture-and-personality schools, which indeed possess unique and valuable concepts which should be used in the analysis.

Dr. George R. Bach (*Psychological Clinic, Kent State University, Kent, Ohio*):

I should like to comment on Dr. Kinsey's remarks concerning the methodological difficulties involved in the interviewing of young children on sexual experiences and attitudes. I should think that the use of standardized projective doll play techniques, such as were used in recently published experiments by me, by Dr. Sears and his associates at Iowa University, and by Dr. Conn, should help to overcome this difficulty in future research.

Some of the results obtained with this technique are pertinent to the present discussion. About 5 per cent of experimentally instigated doll play fantasies of normally adjusted children, ages three to eight, have obvious sexual connotations, even when the experimental situation is kept entirely free from suggestions or questions on the part of the psychologist. This is the average figure, the range being from 0 per cent to about 15 per cent. The percentage of sexual fantasies appears to increase with decrease in mental age, when chronological age is held constant. Also of interest to this discussion, is the finding of very definite sex differences in the doll play fantasies of children. Boys show a greater percentage of sexual and other "taboo fantasies" than do girls, whose fantasies, although more numerous, have a greater percentage of stereotypy and propriety. This difference is noticeable at the age of three and seems to be maintained until early adolescence, beyond which we have not as yet used this technique.

I am inclined to explain these very marked sex differences in terms of early cultural sex-typing and principles of social learning: in other words, cultural influences, such as were brought to our attention by Dr. Bateson's paper.

Mr. Amram Scheinfeld (*New York, N. Y.*):

With regard to the reference just made by Dr. Bach to studies of sex differences in projective doll play, it happens that, by coincidence, a copy of one of these studies was sent to me today, and I have it with me. A statement in this paper is of special significance here, because it emphasizes so strikingly the tendency, in recent years, to assume that where we find early sex differences in behavior or performance, they can hardly be otherwise but culturally or environmentally conditioned. I quote now from the study mentioned, by Pintler, Phillips, and Sears, of the Iowa Child Welfare Research Station:*

> "In two recent studies of projective behavior in pre-school children, one . . . on doll play and the other . . . on Rorschach, indications of sex differences have been found. This has two important implications. First, the clinical interpretation of such behavior must be based on intrasex comparisons; the performance must be viewed against the normative background of the child's own sex rather than against that of all children. Second, a new search must be made for the cultural factors that are presumably responsible for sex differentiation. If marked differences are found as early as ages three and four, it suggests that rather powerful influences must be at work from an extraordinarily early age."

Note those last two sentences. Here, quite plainly, is evidence of the bias which has colored so many similar studies, the assumption, *a priori*, that "cultural factors" are "presumably responsible" for the sex differentiation. And yet, at this conference, we have had considerable evidence of biological factors governing sex differences in behavior in lower animals, suggesting the possibility that sex differences in behavior among human beings might also, at least in part, be influenced by biological factors.

Undoubtedly, a great deal of good has come out of the clarification of the important part played by environment and conditioning in the differentiation of behavior in the human sexes. Nevertheless, I cannot help but feel that there is need for a more objective and less prejudiced approach than we have had in the study of the causative factors for these differences, both among children and adults.

Dr. Emily Hartshorne Mudd (*Marriage Council, Philadelphia, Pennsylvania*):

It is well known to all of us that Dr. Kinsey has stated that he does no counseling and takes no responsibility in aiding in problems of sexual or marital adjustment, during his interviews for his fact-finding survey on human sex behavior. On the other hand, those of us who function in the counseling area have met with varied repercussions among individuals who have been interviewed by Dr. Kinsey or his associates. Such individuals are often helped in adjustment, while sometimes they are quite upset. Further discussion, with a trained person, of the reactions of such persons, when negative after being interviewed for Dr. Kinsey's study, often helps to alleviate difficulties. It is my feeling, and that of other professional persons with whom I have discussed this matter, that no scientist can completely disclaim responsibility for the results of his work. This point has been amply illustrated in the recent discovery of the atom bomb. It seems to many of us that, during interviews requested by Dr. Kinsey for the collection of his material, other processes, usually of therapeutic value, inevitably take place. Dr. Kinsey might have some suggestions to offer to those of us who are involved in further interpretation of the repercussions of his work.

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* **Pintler, Margaret H., Ruth Phillips, & Robert B. Sears.** Sex differences in the projective doll play of pre-school children. J. Psychol. **21:** 73–80. 1946.

Dr. Bela Mittelmann (*Cornell University Medical College, New York, N. Y.*):

The statistical and the psychopathological and case history approach complement each other. However, in some ways, they present contradictions and problems in methodology. Dr. Kinsey's approach is, essentially, a statistical one. He surveys the population and finds a series of gradations, and also a universal or almost universal occurrence of phenomena. He considers all of them as normal variations. Dr. Herman presents the point that the phenomena observed in sick patients, particularly if those phenomena appear and disappear with illness, are to be considered pathological. Or, if those phenomena, being present, distress the patient, and disappear as a result of treatment, those phenomena are also to be considered pathological.

Now, the phenomena with which the statistical approach and the psychopathological and case history approach deal, overlap to a considerable extent. I might give two examples. One is partial or complete frigidity in women. The statistical approach finds a very high percentage of it in the general population. It is not an uncommon observation that a woman who comes for treatment, let us say because of anxiety attacks, or because of depressive reactions or obsessional thoughts, and who also is unable to reach orgasm during intercourse, loses her obvious psychopathological symptoms in the course of psychoanalytic treatment and also develops the ability to reach orgasm, or finds it easier to reach one, and thus the frequency of her orgasm increases. The reverse may happen in the case of very frequent and promiscuous relations. In the course of treatment, the level of sexual drive may diminish, and orgasm is reached during each activity, but the frequency of intercourse diminishes. One could make a construction about the first example that the individual is anxiety- and guilt-ridden, with undercurrent hostility, and therefore incapable of adequate sex relations. The second type of individual constantly has to prove his worth and ability of performance, and also utilizes sex relations to allay discomforts from other sources, such as disappointment, hurt self-esteem, and guilt.

There is no doubt that both methods of approach are essentially valid. However, no adequate synthesis of the two types of results has, as yet, been achieved. The statistical approach can be erroneous, because it does not look to the question of constellation and causation. Every individual has had occasional attacks of common cold, and most people have one or two attacks a year. Yet, there can be no question that the common cold is a pathological state. Or, in some geographical regions, the majority of individuals suffer from pellagra, which obviously does not make pellagra a normal phenomenon. The pathological and case history approach may err in that they consider most of the phenomena occurring in the sick individual as pathological. A comparison with broad distribution of phenomena may be lacking. The ultimate synthesis may be achieved by extensive and detailed individual studies, approaching the scale at which Dr. Kinsey is aiming.

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